assumes that the skater, in fact, glides about on a narrow film of water, the solid turning to water wherever the pressure is most intense, and this water, continually forming under the skate, resuming the solid form when relieved of pressure."

J. Joly.

Geological Laboratory, Trinity College, Dublin.

Mammalian Longevity.

The letter of Dr. Ainslie Hollis in Nature of January 5, on "The Curve of Life," shows that the ratio existing between the periods of maturity and the periods of after-life in various mammals are capable of projection in a regular curve. This led me to inquire if the ratios might not be capable of reduction to a general formula. This seems to be the case, the statement being as follows:—

The full term of life in a mammalian species is equal to ten and a half times the period of maturity divided by the cube root of the period (of maturity), that is

$$\text{f. t. l.} = \frac{10{\cdot}5\,(\text{p. m.})}{\sqrt[3]{(\text{p. m.})}}, \text{ or } 10{\cdot}5 \times (\text{p. m.}),$$

in which f. t. l. is the full term of life, and p. m. the period of maturity.

By the full term of life is meant the period that the animal would live, supposing that its existence were not shortened by enemies, accidents, disease, starvation, overwork or nervous strain; and that it passed out of life by senile decay. This, of course, simply represents an average. It is generally accepted that the period of maturity is best measured by finding the age at which the epiphyses are united to the skeleton. It seems to be about from one and a half times to twice the period of puberty: one and two-thirds and twice seem common proportions. Man, for example, arrives at puberty at about fifteen, and is mature at twenty-five; the lion and tiger arrive at puberty at three years, and are mature at six.

A table is given below, showing the periods of maturity and the full terms of life as obtained from the observations of breeders, scientific men, &c.; and, for purposes of comparison, the full terms of life as calculated by the formula from the same periods of maturity. It will be seen that the two agree as closely as could be expected, especially when we bear in mind the difficulty of fixing with precision the normal life of a species, whose individual members will often die at widely differing ages, from different causes. Hence the different results obtained in many cases by different observers.

The approximation of the results of observation and the formula will be noticed. Blaine on the horse, is from the "Encyclopædia of Rural Sports." He seems to have studied the subject of the horse's age very closely. He says: ". . . a horse of five years may be *comparatively* considered as old as a man of twenty; a horse of ten years, as a man of forty; . . and of thirty-five years, as a man of ninety." Up to ten years of age, then, the horse counts one year proportionately for every four of man, and as man's maturity takes place at twenty-five, this makes the horse's to occur at six and a quarter years. The full term of life given as equivalent to a man of ninety, thirty-five years is almost identical with the result of the formula. Darwin's observations on the elephant are from the "Origin of Species," where he discusses the increase of animals. The other references are from the works of various writers. It should be pointed out in connection with the dormouse, that Dr. Ainslie Hollis gave its full term of life as four years in Nature and four and a half in the *Lancet* of January 21.

Ernest D. Bell.

The editor of Nature has kindly forwarded me Mr. Ernest D. Bell's letter before publication. The formula therein stated is interesting, as it confirms the opinion, given in my previous letter on the subject, that a relationship exists between the duration of adolescence and the length of a mammal's life. Since the publication of the curve of life in Nature, I find that the following domestic animals can be added to those already given. They conform to the requirements of the curve very closely, as may be seen:—

	Observed length of adolescence.	Length of Life.		
		Observed	by curve	Computed by form.
Guinea-pig ...	7 months	6–7 years	7 years	7·33 years
English greyhound	1 year	12 ,,	12 ,,	10·5 ,,
Cat (Mivart) ...	1 ,,	12 ,,	12 ,,	10·5 ,,
Cat (Jennings) ...	2 years	15 ,,	18 ,,	16·67 ,,
Hog	5 ,,	30 ,,	31 ,,	30·7 ,,
English hunter ...	6¼ ,,	35 ,,	35 ,,	35·63 ,,

The age at which growth ceases in man differs considerably in different individuals of the same race. Otto observed all the epiphyses separate in the skeleton of a man, aged twenty-seven years (South's "Pathological Anatomy," p. 126). Such a skeleton could not have completed its growth for another ten or twelve years. The man, had he lived, might have truthfully posed as a youth when he was on the verge of forty. I have in skiagrams observed a difference of upwards of three years in the ages of different subjects, at which osseous union of the epiphyses to the finger-bones was effected. As the age of

Animal.	Observations.				f. t. l. by formula.	Other observations.	
	Authority.	p. m.		f. t. l.		f. t. l.	Authority.
		Months.	Year.	Years.	Years.		
Dormouse	Ainslie Hollis	3	·25	4–5	4·167	—	—
Guinea-pig	Flourens	7	·583	6–7	7·33	—	—
Lop rabbit—							
Buck	R. O. Edwards, p. m.	9	·75	8	8·67	8	Flourens
Doe	,, ,, p. m.	8	·667	8	8·013	8	,,
		Years.					
Cat	St. G. Mivart	1		12	10·5	—	—
Cat	J. Jennings	2		15	16·67	—	—
Goat	Pegler	1·25		12	12·18	—	—
Fox	St. G. Mivart	1·5		13–14	13·76	—	—
Cattle	Ainslie Hollis	2		18	16·67	15–20. 14	Flourens. Gresswell
Large dogs	Dalziel, p. m.	2		15–20	16·67	—	—
English thoroughbred horse	Ainslie Hollis	4·5		30	28·62	—	—
Hog	James Long	5		30	30·7	—	—
Hippopotamus... ...	"Chambers's Encyclopædia"..	5		30	30·7	—	—
Lion	St. G. Mivart	6		30–40	34·67	—	—
English horse—hunter	Blaine	6·25		35	35·63	—	—
Arab horse	Ainslie Hollis	8		40	42·00	—	—
Camel	Flourens	8		40	42·00	40–50	Grindon
Man	Buffon, f. t. l.	25		90–100	89·77	90–100	Flourens
Elephant	Darwin	30		100	101·4	—	—
Elephant	C. F. Holder and Indian hunters	35		120	112·35	120	De Blainville

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twenty-five years for man's cessation of growth is therefore only an average one, in introducing that number as a factor of the curve, I thought that it would be manifestly an error to take examples of exceptionally long lives, when striking an average for length of life. In the curve as published in NATURE (which, although published somewhat before my communication to the *Lancet*, is really a revised curve), I reduced the age of man from eighty to seventy-five years from considerations such as I have just mentioned. Even seventy-five years is somewhat greater than the expectation of life given in Bourne's "Manual," for those who have completed the first half-century of their existence. The age given by Buffon, and quoted by Mr. Bell, is undoubtedly far too long. Similar considerations from some fresh data induced me to reduce the mean lifetime of the domestic mouse from four and a half years to four years, as noticed by Mr. Bell.

W. AINSLIE HOLLIS.

Hove.

Barnes' "Plant Life."

In re centrosomes:

Poor misguided Prof. Zacharias! With absolutely no provocation, he now departs from "the almost universal consensus of opinion among good botanists" by saying of centrosomes (*Bot. Zeit.*, 572: 6, 1899):

"However, on an unprejudiced consideration of the literature involved, one may consider it not impossible that, on renewed search, the centrosomes will finally be again discovered where, for the present ('mark, Jew!'), they have been missed."

And Guignard! What a stupid he is to repeat in greater detail the blunder of figuring and describing those "discredited" centrosomes when all *good* botanists (who swear by Strasburger and his young American students) know that there are no such things! And to think of his calling them "Les centres cinétiques chez les végétaux" (see *Annales des Sci. Naturelles, Bot.*, viii. 5, 177–220, 1898), as though they were common! How "amazingly behind the times"!

But there must be more reason than assigned for designating "Plant Life" as "amazingly behind the times." Prof. Barnes would really be under obligations to the reviewer if he could find time to indicate by number of page or paragraph (doubtless marked as the book was yawned over) the statements to which he considers this phrase applicable. This request is made in all sincerity, and in the hope that the number of these passages will not be so great as to make it presumptuous in its demands upon the reviewer's time.

C. R. BARNES.

THE reviewer cannot help regretting the evident pain which his remarks (vol. lviii. p. 519) have caused Prof. Barnes, though the latter can scarcely seriously believe that his arguments and assertions meet the original objections to which he has taken exception.

Prof. Barnes appears to be particularly aggrieved at the reference made to the figures and account of centrosomes, but his own explanations merely serve to give force to the reviewer's contention that they ought not to have found a place in an elementary book at all.

If the best final reply he can make is to quote the opinion of Zacharias to the effect that "it is not impossible that on renewed search the centrosomes will finally again be discovered," he should see that his case is parlous indeed. He has, in fact, cast a far greater slur on his own critical judgment than the reviewer would have ventured to do. His further quotation of Guignard's recent work might perhaps be regarded as somewhat *ex parte*, even had that investigator reiterated the old statements on which Prof. Barnes' account was based. As a matter of fact he does not do so, and his silence tells against our author.

The somewhat contemptuous reference to Strasburger (who is nevertheless *facile princeps* amongst botanical cytologists) and to those younger American botanists whose reputations, *pacè* Prof. Barnes, are largely founded on the splendid results achieved by them at Bonn, are scarcely calculated to increase one's regard for Prof. Barnes' power of discrimination.

Prof. Barnes appears to be quite unable to realise the degree of mental confusion which would be the inevitable lot of a student endeavouring to deal with the account given by him of the movement of water in plants. In one place (§ 204) root-pressure is spoken of as the force which causes the movement from the root to the evaporating surfaces of the leaves; but in § 207 the author rightly remarks that root-pressure is practically inoperative at the time when transpiration is most active. But he goes on to add that "recent experiments" indicate that the negative pressure of the gas-bubbles in the tracheids may be "a very important, or even the chief factor in lifting the water." After this one ceases to be surprised that no mention is made of the conclusions reached by Dixon and Joly, or by Askenasy!

But Prof. Barnes asks for further evidence for the reviewer's unfavourable opinion of the book. Only a few instances need be mentioned here, for if "this request is made in all sincerity," the author's own friends will easily supply more.

In a work of this kind, it is astonishing to find no mention of the occurrence of motile antherozoids amongst the lower phanerogams, which is perhaps the most important of all recent botanical discoveries—important for the student as clearly showing the connection between the higher and lower plants.

The account given in § 143 of annual rings is so preposterous as to call for no further comment.

The respiratory quotient of the ordinary plant is still given as unity, when, as a matter of fact, it is nearly always other than 1 in growing plants.

The statement that "true geotropic curvatures are brought about by the acceleration of the growth of the irritable cells" is, as it stands, absurd, for it involves no necessary *curvature* at all.

A student reading the account given in § 230 would naturally fall into the error of concluding that all the rays of light absorbed by chlorophyll are equally active in promoting assimilation.

In view of the evidence here adduced, at Prof. Barnes' own request, the reviewer considers that his judgment of the book was by no means unduly harsh or severe.

THE REVIEWER.

Optical Experiment.

BEING driven past a row of trees, I noticed that their intermittent shadow on the closed eye-lids gave rise to a vivid chess-board pattern of red and black squares arranged horizontally and vertically. These were perfectly regular, each being equal to about one-twelfth of an inch at ten inches distance. Waving the open fingers in front of the closed eye-lids exposed to the sun gave the pattern fairly well, but better by flashing the sun's rays across the lids by means of a vibrating hand-mirror. I see about seven or eight squares each way, the outer ones not well defined; but a younger man, who was not told what to expect, described them as more numerous.

What structure in the eye gives rise to the phenomenon? It is not caused by the eye-lids, because a piece of tissue-paper can be substituted, the eyes then being open. If the paper is white the squares are white and black. The pattern occupies the centre of the field of each eye.

THOM. D. SMEATON.

Adelaide, South Australia, February 6.

A SEISMOLOGICAL OBSERVATORY AND ITS OBJECTS.

TEN years ago seismologists practically confined their attention to the movements of the ground which could be felt. In Italy and Japan, where these were frequent and sometimes violent, they attracted serious attention; whilst in Britain, where earth tremors were comparatively unknown, any suggestion that this country should establish a seismological observatory might only have cast doubts upon the mental balance of its author. At that time it was popularly supposed that in our islands earthquakes were of such rare occurrence that a special establishment for seismological investigations was unnecessary. Seismology, however, like several other sciences, has in a comparatively short period advanced with strides, and now stands as foster-mother not only to a Romulus and Remus, but also to a number of other children all filled with promise.

Now we know that in England, or in any other non-seismic region on the surface of the globe, at least seventy unfelt earthquakes, each of which have durations varying between twenty minutes and several hours, may be recorded yearly. The probability is that these movements are transmitted from their origins as compressional

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